CENTER FOR INSTITUTIONAL REFORM AND THE INFORMAL SECTOR

University of Maryland at College Park

Center Office: IRIS Center, 2105 Morrill Hall, College Park, MD 20742
Telephone (301) 405-3110 • Fax (301) 405-3020

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James E. Rauch
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Author: James E. Rauch, University of California at San Diego

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James E. Rauch*

Department of Economics
University of California, San Diego
La Jolla, CA 92093

and

National Bureau of Economic Research
Cambridge, MA 02138

Recent work in the sociology of economic development has emphasized the establishment of a professional government bureaucracy in place of political appointees as an important component of the institutional environment in which private enterprise can flourish. I hypothesixe that establishment of such a bureaucracy will lengthen the period that public decision makers are willing to wait to realize the benefits of expenditures, leading to allocation of a greater proportion of government resources to long-gestation period projects such as infrastructure. I also hypothesize that this increased government investment in inputs complementary to private capital will increase the rate of economic growth. These hypotheses can be tested using data generated by a "natural experiment" in the early part of this century, when a wave of municipal reform transformed the governments of many U. S. cities. Controlling for city and time effects, adoption of Civil Service is found to increase the share of total municipal expenditure allocated to road and sewer investment. This share in turn has a positive effect on growth in city manufacturing employment in a model with time effects and city random effects but not in a model with both time and city fixed effects .

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I. Introduction

Recent analyses of economic policy-making in less developed countries (LDCs) have stressed that the individuals who make up the state apparatuses can to some extent act independently, rather than responding passively to voters or interest groups as in much of the political economy literature. Such a state might be expected to exhibit the "predatory" behavior predicted by writers such as Lal (1988), as each state functionary seeks to implement regulations on private sector economic activity that will maximize the bribes he can extract. Indeed, we do observe such purely rent-seeking states in LDCs. A good example is Zaire, of which President Mobutu has stated "holding any slice of public power constitutes a veritable exchanged instrument, convertible into illicit acquisition of money or other goods" (Young 1978, p. 172). What is remarkable is that some LDC governments do not act as predators. In East Asia, for example, the Korean and Taiwanese states have worked hand in glove with the private sector to promote investment and enhance the capacity of private firms to enter international markets (Amsden 1989, Wade 1990), earning these governments the moniker "developmental states".

In his comparative analysis of the role of the state in the development of several LDCs, Evans (1992) argues that professionalization of the state bureaucracy is a necessary (though not sufficient) condition for a state to be "developmental". He suggests a set of measurable state characteristics that can be used in empirical analysis to quantify bureaucratic professionalization or "Weberianism". These include percentage of bureaucratic positions filled by civil service exam rather than political appointment, percentage of those taking the civil service exam who pass it, and average length of tenure in a given government department (evidence of stable career-building as opposed to rent-seeking opportunism). The potential impact of Weberian bureaucracy on economic development is twofold. On the one hand, the negative effect the state can have on growth by taxing the returns to private investment could be lessened by minimizing the implicit taxation caused hy rent-seeking. On the other hand, the pnsitive role that the state can

play in providing complementary inputs for the private sector could be enhanced since the long gestation periods of infrastructural projects are well suited to bureaucrats pursuing career building within the government departments overseeing the projects. The empirical considerations described in the next paragraph led me to defer investigation of the former hypothesis to future work and focus on the latter hypothesis here.

During the Progressive Era in the United States (roughly the first two decades of the twentieth century), a wave of municipal reform transformed the governments of many cities. Broadly speaking the reforms can be seen as attempts to move away from predatory to more Weberian state characteristics. In this paper I investigate the potential positive impact of these reforms on city growth through the mechanism of increased allocation of government budgetary resources to long-gestation period investments such as road and water systems. I believe that these events in the history of U. *S. cities* provide a natural experiment that allows me to avoid two problems characteristic of cross-*country* empirical work on the political economy of growth and development.[1] A nearly universal problem in this literature is that the policy variable determined by the theory expounded in the paper is unobserved.[2] Frequently the theories suggest that a characteristic of the state or the society at large affects a rate of explicit or implicit taxation through a political decision-making process, and that this rate of taxation in turn affects the rate of long-run economic growth. Data on the rate of taxation in question, however, are never presented. By examining the share of municipal expenditure allocated to investment in infrastructure, for which data are available, I avoid this first problem. The second problem with this work is that the empirical analysis is either purely cross-sectional or does not use the time series

[1] Use of city, state, or regional data to make inferences about the mechanisms of economic growth has become increasingly popular, as exemplified by work such as Glaeser et al. (1992), Barro and Sala-i-Martin (1992), and Rauch (1993a, 1993b). One reason for this popularity is that explanatory power is enhanced because localities within a single country differ from each other along many fewer dimensions than do countries themselves.

[2] Exceptions have begun to appear very recently, including Alesina and Perotti (1993) and Cukierman et al. (1993).

variation in the data to control for unobserved cross-sectional characteristics that could influence the dependent variable and be correlated with the state or social characteristics of interest.[3] My natural experiment allows me to avoid this problem by generating variation in my selected state characteristics over time. To my knowledge the present paper is unique in this literature by virtue of having both an observed policy variable and a fully utilized panel structure.

The plan for the remainder of this paper is as follows. Section II sketches a model of political decision-making, infrastructure investment, and economic growth in cities. The following section describes the construction of the data set that is used to test the model. Section IV gives results, and section V concludes.

II. Bureaucracy, infrastructure, and growth in cities

In this section I sketch the formal model developed in Rauch (1994) of the effects of a stylized reform on the allocation of city expenditure and on city growth. The extent to which actual reforms matched the stylized reform will be discussed at the beginning of the next section. The mechanism of growth is modeled very simply so that attention can be focused on the political decision-making process. Investment in new infrastructure is assumed to generate city growth by providing a complementary input that attracts investment of private capital in traded goods industries (manufacturing), creating jobs that in turn attract migrants from a surrounding agricultural hinterland. A national capital market and productivity in the agricultural hinterland fix the return to private capital and the wage rate, respectively. Employment, the private capital stock, and output (manufacturing value-added) then all grow at the same rate as the stock of infrastructure.

City politicians can obtain benefits from both the inputs to the municipal production process and its outputs. Examples of benefits from the former would be

[3] A notable exception is De Long and Shleifer (1993).

patronage jobs and kickbacks while examples of benefits from the latter would be voter satisfaction with municipal services and enhanced prestige and power from city growth generated by new infrastructure. In the model below the decision concerning allocation of **expenditure to production of different outputs is driven by the benefits these outputs yield.** To disentangle these benefits from the benefits yielded by inputs I need to assume that benefits from inputs are unaffected by the composition of output, so that a dollar of expenditure on provision of current services creates as many patronage jobs (for example) as a dollar of expenditure on new infrastructure. I abstract from the current *level* of expenditure by making assumptions that effectively remove it from political control: fees charged for the use of services generated by the current stock of infrastructure are the only source of current government revenue, and the current stock of infrastructure is inherited from the previous period.*

City government must decide how to allocate these fees between expenditures that **yield immediate benefits (e.g., police and fire protection, filling potholes)** and investment (new infrastructure). Its decision is modeled as the outcome of a "Principal-Agent" relationship: the Principal (e.g., the city council) employs the Agent (e.g., the city planning bureaucracy) to identify needs for current and capital expenditure and then allocates funds accordingly, the disbursement of which is overseen by the Agent. The Principal faces an uncertain prospect of reelection or election to higher office. Before municipal reform, the Agent is assumed to be a political appointee whose probability of retaining office is identical to that of the Principal, while after reform the Agent is assumed

*Historians and political scientists writing about the fiscal impact of municipal reform, on the other hand, have focused on the level of expenditure (the "standard yardstick" according to Brown and Halaby (1984, p. 70)) rather than its allocation. The problem with this approach in my view is that in predicting the effect of reform on the level of municipal expenditure there is no reasonable assumption that allows one to separate the benefits from inputs and outputs. Opportunities for corruption, broadly defined, are linked to the level of expenditure, but so is the amount of public services that a city can provide. There is no reason to think this amount should be above the "optimal" level prior to reform, especially in the pre-World War II **era.** In this section I take an agnostic view concerning both the effect of reform on corruption and its effect on the level of public services relative to its optimum.

to have lifetime tenure and can be terminated only for just cause (e.g., as determined by a Civil Service Commission).[5]

Investments in infrastructure do not pay off until the next election cycle. This leads to a difference in the importance placed on capital versus current expenditure between the Principal and the post-reform Agent. Why do the Agent's preferences matter at all? The answer is that he can use his powers of information collection and expenditure oversight to manipulate the Principal. For example, in preparing an itemized budget for submission to the Principal he can spend more effort identifying attractive infrastructure projects relative to identifying pressing needs for current expenditure than the Principal would in his place, and/or he can put more effort into overseeing the execution of investment relative to current expenditure than the Principal would in his place. It is assumed, however, that the Principal can imperfectly monitor the Agent's actions so that there are limits to how far the Agent can pursue his own preferences at the expense of the Principal%. The resulting division of budgetary resources between current and capital expenditure will therefore lie somewhere in between the Principal's and the Agent's ideals. In Rauch (1994), utility functions and a monitoring "technology" are specified that generate steady state outcomes for the share of expenditure allocated to infrastructure investment within an overlapping generations framework.

The stylized reform therefore increases the share of expenditure allocated to infrastructure investment and consequently the steady-state growth rate for the city. Another interesting result of reform in this model is a reduction in the probability that the Principal will remain in power due to lower voter satisfaction with the delivery of current municipal services. This suggests that reform reduces the power of political machines, which is one of the objectives ascribed to municipal reformers by historians and political

[5]Reform thus provides municipal government with the "bureaucratic insulation" from the political process often cited by analysts as crucial to LDC state effectiveness in managing economic adjustment (see, e.g., Haggard and Kaufman (1992, p. 20)). The model of this section can be seen as showing how this insulation can make the state more effective in promoting long-term growth as well.

scientists. However, the cause of this result is lower provision of current services, presumably making current city residents worse off. Why, then, would they have voted to enact reform in the first place? One possible answer is the thesis of Hays (1964) and Weinstein (1968), which can be summarized as follows: (1) During the Progressive Era, large scale corporate organization was coming into its own in the U. S. private sector; (2) local businessmen came to see the city as a large corporation and wanted to make it function more like one; and (3) they led the drive for reform, using popular discontent with political machines to win the voting majorities needed to enact their agenda. If this is true, enactment of reform was associated with a change in the "hegemonic group" within the city. Suppose that this group wanted to use the reformed municipal government to promote growth through investment in infrastructure. In this case the cause of any observed change in the share of city expenditure allocated to this investment would be reform per se rather than the content of the reform as I have argued.6 Fortunately, it will prove possible to deal with this problem effectively in the empirical analysis, to which we now turn.

III. The Data Set

Three reforms that radically changed the structure of municipal government were undertaken during the Progressive Era. Civil Service, also known as the merit system, was introduced to the United States in the 1880s but did not really take hold at the municipal government level until the 1890s and the Progressive Era. It required that applicants for city employment pass exams in order to be considered and that they could be fired only for

[6] Another possible argument for why reform per se could have increased the infrastructure share of expenditure is that reform improved the city's access to financing. A simplifying assumption made in the above model is the lack of access to capital markets by the city. In fact, debt ceilings expressed as percentages of assessed valuation were imposed on cities by the state governments that guaranteed their general obligation bonds. It is possible that reform could have been used successfully by cities to make a case for raising their debt ceilings. On the other hand, one of the major stated aims of many reformers was to reduce municipal indebtedness.

just cause. It is the most essential element of "Weberian bureaucracy". The two other structural reforms were introduced during the Progressive Era. The Commission form of government was introduced in Galveston, Texas in 1901. Under this form of government the mayor, city council, and any other elected officials were replaced by a group of "commissioners", typically five in number, each of whom had both executive and legislative powers for a different department of municipal government. The City Manager form of government was introduced in Staunton, Virginia in 1908. Under this form of government all executive powers were concentrated in a single appointed official called the city manager who was answerable to the city council, of which the mayor became merely the most important member. The city manager did not have legal protection against being discharged by the city council (though sometimes he was entitled to a public hearing on written charges), yet at the same time the proponents of the city manager system clearly intended that he have lifetime tenure.

The stylized reform in the model of the previous section was clearly based on the institution of Civil Service. However, it is not clear to what extent the part of the bureaucracy that is responsible for identifying and supervising projects was covered, although the Civil Service Assembly pamphlet cited below does indicate when coverage is limited to police and/or fire department employees. The City Manager reform created an individual position that perfectly fits that of the post-reform Agent except for the absence of legal protection for lifetime tenure. In their survey of forty-eight U. S. cities, Stone, Price, and Stone (1940, pp. 63-65) found that, "Some cities had a high rate of turnover in managers," one of the causes of which was "frequent political changes in the councils which resulted in the dismissal of the managers." On the other hand, in twenty-three cities

> the city manager's job was one of permanent tenure. Some managers had resigned to accept better positions in private business or as managers in other cities, and some had died in office, but no city manager was discharged or was forced to resign.... Twenty-two city managers in the forty-eight cities studied served a single city for ten or more years.

Of course this long tenure could simply reflect the complete harmonization of preferences

between the city council and the city manager that prevails for the Principal and the pre-reform Agent in the model of the previous section. Thus while the model predicts that Civil Service will have a positive effect on the share of infrastructure investment in total municipal expenditure, the prediction for City Manager is unclear. In contrast to Civil Service and City Manager, the Commission reform was wholly unrelated to the stylized reform in the model above. Commissioners were elected officials, so this reform is not predicted by my model to change the allocation of resources between capital and current expenditure. The Commission form of government therefore serves as a kind of control: its adoption should not have the same effects as the other two reforms. If it is in fact found to have the same effects, one can argue that city investment and growth are responding to reform per se rather than to the content of the reform.

Data on municipal government reform was gathered from several sources. A pamphlet entitled. Civil Service *Agencies in the United States: A* 1937 Census, published by The Civil Service Assembly of the United States and Canada, gives the dates of adoption of Civil Service in U. S. cities. Unpublished work by M. Craig Brown gives the dates of adoption (and discontinuation, if applicable) of the Commission and City Manager forms of government, which I checked against the original sources to the extent possible.[7] A dummy variable was established for each reform that takes the value one during the years when the reform is present for a city and zero during the years when it is absent. Civil service coverage that is limited to police and/or fire department employees is assigned a value of zero.

Partly in response to the municipal reform movement, the Bureau of the Census began collecting city budget data in 1902 and publishing it in a standard format for the express purpose of allowing cross-city comparisons. *This* was done in *Statistics of Cities* and *Financial Statistics of Cities* for all cities with population of 30,000 or more through

[7]The original sources are given in the Appendix to Brown and Halaby (1984) and include contacts with city clerks.

1931, after which only cities of 100,000 or more were covered. Included among the dozens of series published each year are total expenditure, total capital outlays, and the infrastructural components of capital outlays: roads, sewers, and water supply, where all waterway and port improvements are included in roads.[8] Thus we can use this data to compute the share of city expenditures devoted to overall investment and to infrastructure investment more specifically.

From Census of Population and Census of *Manufactures* manufacturing employment and value-added data are available from 1899 to 1929 at five-year intervals for cities with population of 10,000 or more, after which the data is only available for cities of population 100,000 or more. Since manufacturing is a "base" industry with a "multiplier" effect its growth should be a good proxy for overall city growth. Use of population growth itself is problematic because it contains a large exogenous demographic component and is estimated during intercensus years without the benefit of additional surveys. These estimates will be most inaccurate precisely when net migration is large, and indeed complete population data is not even available for some rapidly growing cities in the sample described in the next paragraph. For this reason the share of infrastructure investment in total expenditure rather than infrastructure investment per capita is-used in the analysis below.

In order to maximize city coverage during the Progressive Era when reform was most common I chose to examine the period 1902-1931 only. During this period city

[8]Here I define as infrastructure the same components of capital outlays selected by Eberts, Dalenberg, and Park (1986). Holtz-Eakin (1992) includes other public utilities with water supply, but use of this slightly broader definition turns out to have no qualitative impact on the results presented in section IV below. In both papers the primary concern was to construct estimates of the public capital stock to be used in estimating aggregate production functions. It could be argued that the stock of *human* capital should be included in our definition of the stock of infrastructure, so that *current* expenditure on education that adds to the skills of the future labor force should be added to capital expenditures on roads, sewers, and water supply to get a comprehensive measure of infrastructure investment. There are two problems with this approach. First, students may not join the labor force of the city in which they were educated, so expenditure on their education may not all augment next period's municipal infrastructure stock. Second and more importantly, numerous studies have shown that educational expenditure per student is treated as an amenity by current city residents (voters), so the effect of reform on the share of budgetary resources allocated to current educational expenditures is ambiguous.

financial statistics were not collected for 1914 or 1920, were incompletely collected in 1921, and were collected with insufficient detail to distinguish all three infrastructure investment categories (roads, sewers, and water supply) from other investment expenditures in 1902, 1903, 1913, and 1922. Thus a maximum time series of 23 years is available. ***Statistics of Cities*** supplies data for 150 cities with population greater than 30,000 in 1904. Of these, 144 had complete financial and reform data for all 23 years. [9]

I computed two different ratios of infrastructure investment to total expenditure. The first, RSW, uses the standard definition of infrastructure investment (roads + sewers + water) in the numerator and covers 65 percent of total investment expenditure on average. (All definitions are repeated in Table 1 for easy reference.) However, 16 of the 144 cities in the sample never recorded positive expenditure for water investment, indicating they did not own their water utilities, and 29 of the 144 cities did not record positive expenditure for water investment for more than half of the years in the sample (12 or more). I therefore computed RS using roads + sewers only in the numerator, which covers 52 percent of total investment expenditure on average. (None of the 144 cities recorded zero expenditure for either road or sewer investment for 12 or more years.) The sample means and standard deviations for RSW and RS are reported in Table 2a. We also learn from Table 2a that there is within-sample variation over time in Civil Service for 40 cities, in City Manager for 19 cities, and in Commission for 59 cities. The rate of discontinuation of Commission is higher than for City Manager, while Civil Service is known to have been dropped in only one case (the aborted reform period 1913-1916 in Denver).

Manufacturing employment and manufacturing value added data are available from 1899 to 1929 at five-year intervals for all but three of the 144 cities. When explaining the

[9] Two cities were dropped due to contradictory data on the presence of Commission government, two cities fell below 30,000 population at some point before 1931, one city was consolidated with another, and Washington, D. C. was dropped because its municipal government is controlled by the U. S. Congress.

growth of manufacturing employment and value-added in a panel, however, we will begin in 1904 since this is the first year for which we have infrastructure investment data. For this reason, in Table 2b summary statistics are given for five-year growth rates the first of which is for the period 1904-1909.

IV. Results

Before turning to the investigation of the effects of reform on infrastructure investment that is the central concern of this paper, we should ask whether our measures of infrastructure investment are important and meaningful. It could be that most city expenditure on infrastructure investment goes to create "white elephants" that are useless for private sector production. It could also be that the census officials who consolidated and standardized the municipal accounts did a poor job, or that the accounts with which they worked were badly misclassified to begin with. These concerns lead me to do some preliminary cross-sectional regressions of growth on investment modeled on the cross-country growth regressions that have become so popular in the literature. Levine and Renelt (1992) note that a positive and highly significant effect of the investment share of GDP on the growth of GDP per capita has been consistently found in the literature and that this effect is very robust to the inclusion of other right-hand side variables in cross-country growth regressions. We might therefore expect analogous findings for the effect of the infrastructure investment share of expenditure on manufacturing employment and value-added growth in our city sample. The other right-hand side variables available to us are the 1899 levels of manufacturing employment and value-added (analogous to the initial level of per capita GDP in cross-country growth regressions), dummies for the U. S. Census regions (analogous to the continent dummies in cross-country growth regressions), and time averages of the reform variables themselves.

The results using 25-year growth rates for the period 1904-1929 and time averages of RSW and RS for the same period are shown in Tables 3 and 4 for employment growth

and value-added growth, respectively. The association of both measures of infrastructure investment with city growth is highly significant and robust. Perhaps most remarkable is the comparison of columns (4) and (8) with column (9) in Table 3, showing that the strong regional association with employment growth becomes insignificant (at the five percent level) when infrastructure investment is included in the set of right-hand side variables. Tables 3 and 4 also reveal a tendency for cities with large initial manufacturing sectors to grow more slowly, as indicated by negative coefficients on the logs of 1899 employment and value added, though for value added this tendency is very weak. There is also a tendency for the city manager form of government to be associated with higher growth, especially of manufacturing value added, even after controlling for the share of infrastructure investment in municipal expenditure. The main conclusion from these partial correlations is that the investment expenditure ratios are measuring what we are interested in because their association with growth is so strong, though of course no causal inferences can be made.

Having established that our investment expenditure ratios are worthy subjects for investigation, I now proceed to attempt to determine if and how they were affected by structural municipal reform. To begin, I use the data described in Table 2a to estimate a standard fixed effects model (one including both city and time dummies) with RSW and RS as the respective dependent variables and CIVSER, MANAG, and COMMISS as the explanatory variables. As I mentioned in the introduction, city fixed effects are crucial in order to control for unobserved cross-sectional characteristics that could influence the dependent variable and be correlated with the explanatory variables. The use of time dummies is equally important in the present instance because most reforms were not rescinded, so that investment rates later in the period are much more under the influence of reform than in the early part of the period. In the absence of time dummies estimates will be biased if there are systematic differences between the earlier and later parts of the period, for example due to macroeconomic conditions or even changes in census accounting

procedures.

The first and second columns in Table 5 report the results of these fixed effect regressions. As predicted, each infrastructure investment ratio tends to be higher on average with civil service than without, the effect being stronger for the share of expenditure allocated to road and sewer investment. On the other hand, each infrastructure investment ratio tends to be lower on average with the city manager and commission forms of government than without, the effect being much stronger for COMMISS. It is straightforward to infer from these two regressions that if we use the share of expenditure allocated to water supply investment as our dependent variable the coefficient on Civil Service will be negative. This is confirmed by the third column of Table 5. As I noted above, 20 percent of the cities in the sample apparently did not own their water utilities during all or most of the sample period, so one can argue that the result for roads and sewers is a better indicator than the result for water of the impact of Civil Service on investment in infrastructure. A source of fnrther evidence pertaining to this argument is the effect of adoption of civil service on noninfrastructure investment. 'I'he model of section 11 makes no prediction regarding this effect, but if it were negative it would suggest that the positive effect of Civil Service on road and sewer investment is an aberration. In fact, the fourth column of Table 5 shows that the share of expenditure allocated to noninfrastructure investment tends to be higher on average with civil service than without.

In the discussion in the introduction to this paper I argued that investment in infrastructure is distinguished by its long gestation period. This gestation period has at least two components, a planning period and a construction period, either of which could extend over more than one year. This should create persistence in infrastructure investment expenditure due to difficulty in terminating projects that are "in the pipeline", on the one hand, and slowness in bringing new projects "up to speed", on the other hand. An econometrically natural way to handle this inertia is to use the partial adjustment

model. For the reader's convenience I will repeat this standard model here. The desired level of the dependent variable y (e.g., RSW or for city j in year t is determined by

$$y^*_{jt} = \alpha_j + \gamma_t + x_{jt}\beta, \quad (1)$$

where α_j is a city fixed effect, γ_t is a year fixed effect, and x_{jt} is a vector of explanatory variables (e.g., municipal government reforms). The adjustment of the actual level of y is a proportion of the difference between this year's desired level and last year's actual level:

$$y_{jt} - y_{jt-1} = \lambda(y^*_{jt} - y_{jt-1}) + \epsilon_{jt}, \; 0 < \lambda < 1, \quad (2)$$

where ϵ_{jt} is a Gaussian white noise error term. Substituting equation (1) into equation (2) and rearranging yields

$$y_{jt} = (1 - \lambda)y_{jt-1} + \lambda\alpha_j + \lambda\gamma_t + x_{jt}\lambda\beta + \epsilon_{jt} \quad (3)$$

Note that when estimating equation (3) we can recover estimates of the parameters in equation (1) by dividing the estimated coefficients on the city and year dummies and the explanatory variables by one minus the estimated coefficient on the lagged dependent variable.

The estimates of equation (3) using RSW and RS as dependent variables are reported in the fifth and sixth columns of Table 5, respectively. (The estimates of equation (3) using WATER and NONINFRA as dependent variables, not reported here, are similar to those for the third and fourth columns of Table 5 but with all coefficients smaller and no changes in sign.) As shown by Nickell (1981), the estimated coefficients in a fixed effects model with a lagged dependent variable are subject to an asymptotic bias of order 1/T, where T is the number of time periods in the sample less the number of lags. Since in our case T = 22, this bias should be small and I will ignore it. Taking the coefficients and standard errors at face value, then, we see that adoption of civil service has a positive and statistically significant (at the five percent level) effect on the share of expenditure allocated to road and sewer investment. The point estimate of the effect of CIVSER on RS in equation (1) is $0.0120/(1 - 0.4528) = 0.022$. To get an idea of whether the effect of CIVSER on RS is "large" or "small", we can allow ourselves a causal interpretation of the

coefficient of 0.21-0.23 on RS in the regressions in Table 3. The adoption of civil service then results in an increase in the manufacturing employment growth rate through the channel of increased infrastructure investment of roughly 0.005 or more than 25 percent of its mean value.

Moving to the effects of the other structural municipal reforms, recall that the prediction in section III for the effect of the city manager form of government on the allocation of expenditure to infrastructure investment was either positive or zero. Arguably, the finding of zero effect in the fifth and sixth columns of Table 5 reflects the success of this reform from the point of view of its inventor, who wanted municipal affairs to be administered by a professional yet for his administration to embody the will of the city council.[10] The effect of the commission form of government on the allocation of expenditure to infrastructure investment is not as predicted, being negative rather than zero. Nevertheless, even this finding helps us to state with confidence that the content of reform rather than reform per se was the important determinant of its effect on expenditure allocation.

Returning to the thesis mentioned at the end of section II, perhaps in the cases where the city manager and commission reforms were rescinded the cause was the failure of the municipal government to implement the increase in the share of expenditure allocated to infrastructure investment that the backers of reform expected. Under this hypothesis the zero and negative effects on infrastructure investment of the city manager and commission reforms, respectively, might be attributable to the presence of "bad" city manager and commission governments in the sample. To test this hypothesis I created a dummy variable indicating whether a city rescinded a city manager (commission) reform, interacted it with MANAG (COMMISS), and then included this interaction term in the

[10] Stone, Price, and Stone (1940, p. 14) describe the intent of the inventor of the city manager plan (Richard S. Childs) as follows: "By authorizing the council to hire and fire the city manager at its discretion, however, the city manager plan effectively gave the council control over administrative, as well as over legislative, policy."

panel regressions. A negative coefficient on MANDROP (COMDROP) indicates that "bad" city manager (commission) governments had a more negative or less positive effect than the rest of these governments on the share of municipal expenditure allocated to infrastructure investment. In fact, the coefficients on both MANDROP and COMDROP in the last two columns of Table 5 are positive (though statistically insignificant). If anything, these results suggest that voters might have rescinded the reforms in these cities because the governments allocated *too much* expenditure to infrastructure investment, thereby displaying the preference for provision of current services assumed by the model of section II.

We now turn to an investigation of the relationship between the infrastructure investment share of municipal expenditure and city growth in a panel. I estimate a model where manufacturing employment or value-added growth in any five-year period is a function of the infrastructure investment share of expenditure averaged over those five years and the log of the initial level of employment or value-added. For example, the rate of employment or value-added growth for the period 1924-1929 is a function of RSW or RS averaged over the five years 1924-1928 and the log of the level of employment or value-added in 1924. Both right-hand side variables are clearly endogenous, so I instrument for them using their lags and five-year averages of CIVSER, MANAG, and COMMISS. Unfortunately, estimation of this instrumental variables model with city fixed effects creates a problem in our short panel (T = 4) because the fixed effects induce a time-averaging of the error term, invalidating the use of lagged endogenous variables as instruments. One solution to this problem is to difference out the fixed effects and then estimate the differenced model using instrumental variables. We must then instrument for, say, the difference between RSW in 1924-1928 and 1919-1923 using RSW in 1914-1918 or, better yet, the difference between RSW in 1914-1918 and 1909-1913. Not surprisingly, this procedure yields only statistically insignificant coefficients on the explanatory variables.

Faced with these difficulties I decided to abandon the city fixed effects model in favor of a city random effects model. Table 6 reports the results of instrumental variables regressions of manufacturing employment or value-added growth on RSW or RS and period dummies, without and with city random effects.[11] We see that the strong similarity between the cross-sectional results for employment and value-added growth, evident from a comparison between Tables 3 and 4, disappears in the panel regressions. For employment growth, the coefficients on the share of municipal expenditure allocated to infrastructure investment are not only consistently positive and significant (though only at the ten percent level for RS in the random effects specification), but they are larger than in the cross-section regressions. The coefficients on the initial value of the log of employment are consistently of the expected sign but insignificant. For value-added growth, none of the coefficients on the share of municipal expenditure allocated to infrastructure investment are significant, and the coefficients on the initial value of the log of valuc-addcd arc consistcntly insignificant and *not* of the expected sign. Wc arc lcft with the unsatisfactory conclusion that infrastructure investment positively affects growth in a city's manufacturing employment but not growth in its manufacturing value-added. It may simply be that our time series on manufacturing value-added are too noisy to be informative. Indeed, if we are willing to cut the number of observations in half and use ten-year growth rates instead of five-year growth rates, without city random effects we obtain coefficients on RSW and RS for value-added growth that are positive and significant at the one percent level, though they become insignificant when city random effects are included.

[11]The estimates in the columns with city random effects were computed as follows. The consistent (absent any problem with omitted variables) but inefficient coefficient estimates in the corresponding column without city random effects were used to generate the estimated covariance matrix that in turn was used to transform the data in a feasible GLS procedure. The method of instrumental variables was then applied to the transformed data. This is the "IV-GLS analog" estimation recommended by Bowden and Turkington (1984, Chapter 3).

V. Conclusions

The institution of Civil Service in U. S. cities during the Progressive Era was found to have a positive effect on the share of municipal expenditure allocated to investment in infrastructure. I have interpreted this central finding as showing that professionalization of the state bureaucracy lengthens the period that public decision makers are willing to wait to realize the benefits of expenditures, leading to allocation of a greater proportion of government resources to long-gestation period projects such as infrastructure. The share of municipal expenditure allocated to investment in infrastructure was in turn shown to have a positive effect on growth in city manufacturing employment but not on growth in city manufacturing value-added. By finding an observable policy variable that my theory predicted would be affected by a change in a particular state characteristic and that would in turn affect economic growth, I have tried to put my theory to a much more direct test than has typified the literature on the political economy of growth and development.

If my interpretation of my central finding is correct, it should be corroborated by case studies of the role of professional bureaucracies in economic policy-making. I know of two such studies for U. S. urban areas during the Progressive Era, one by Doig (1988) for the Port of New York Authority and the other by Erie (1992) for Los Angeles. These studies have focused on quasi-independent agencies headed by boards of commissioners who are *appointed* to *fixed, overlapping* terms, giving the Guard as a whole an "insulation" and permanence approximating that of the post-reform Agent of section II. Doig states explicitly that the Port of New York Authority had "some advantages in vision--some ability to see further than the next election" (p. 83). Although the Authority was created in 1921 to rationalize the operation of the Port of New York, its vision led it "to turn to motor-vehicle bridges in the late 1920s and construct the George Washington span and three smaller bridges--in less time than expected, at lower cost, and without the interstate conflicts that marred and delayed important projects by other agencies" (p. 86). In Los Angeles, after civil service was adopted in 1903 and a slate of reform candidates was elected

in 1906 (although neither a city manager nor a commission charter was ever passed), the city government, created a harbor commission (without a harbor) and a water and power commission. According to Erie, these "bureaucratic machines" were the driving forces behind the realization of "massive public projects [that] supplied the three essential pillars of regional development--the man-made harbor at Wilmington-San Pedro (the largest in the world), the Owens River and Colorado River aqueducts (among the most complex engineering feats of their age), and the Department of Water and Power's hydroelectric plants (the largest municipal power system in the nation) generating the cheap energy needed to attract Eastern industry after World War I" (p. 520). It remains to be seen whether case studies of other cities or countries will show professional bureaucrats to be key proponents of long-term productive investment.

An underlying point made both by these case studies and by my statistical analysis is that institutions, such as appointed commissions and civil service, can matter for economic development. In this connection it is worth noting that Brown and Halaby (1984, p. 77), in their study of machine politics and urban reform movements in thirty large U. S. cities during the period 1890-1940, found "that reformers consolidated city power so seldom and so briefly" that it was necessary to turn "to the structural byproducts. of reform" in order to measure the impact of reformers against that of "bosses". This suggests as a subject for future research an interesting model of the reform process, where a reform party can have a lasting impact even if it holds power only for a short time provided that it implements institutional changes that are "sticky". As one of the major historical episodes of successful administrativc rcform in any country, thc Progrcssivc Era will continue to be a fertile source for investigations of the economic impact of political institutions and the determinants of institutional change.

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Table 1: Definitions of Variables in Tables 2-6

RSW: Ratio of road + sewer + water investment expenditure to total city expenditure (time-averaged in Tables 3, 4, and 6)

RS: Ratio of road + sewer investment expenditure to total city expenditure (time-averaged in Tables 3, 4, and 6)

WATER: Ratio of water investment expenditure to total city expenditure

NONINFRA: Ratio of noninfrastructure investment expenditure to total city expenditure = all investment expenditure/total city expenditure - RSW

CIVSER: Dummy variable for presence of civil service system not limited to police and/or fire department employees (time-averaged in Tables 3 and 4)

MANAG: Dummy variable for presence of city manager form of government (time-averaged in Tables 3 and 4)

COMMISS: Dummy variable for presence of commission form of government (time-averaged in Tables 3 and 4)

MANDROP: MANAG x dummy variable for city that dropped city manager form of government

COMDROP: COMMISS x dummy variable for city that dropped commission form of government

EMPGR: 5-year growth rate of manufacturing employment

VALGR: 5-year growth rate of manufacturing value-added

MIDWEST: Dummy variable for Midwest U. S. Census region

SOUTH: Dummy variable for South U. S. Census region

WEST: Dummy variable for West U. S. Census region

LEMP99: Log of 1899 level of manufacturing employment

LVAL99: Log of 1899 level of manufacturing value-added

LEMPINIT: Log of level of manufacturing employment at beginning of 5-year period

LVALINIT: Log of level of manufacturing value-added at beginning of 5-year period

Table 2a: Summary Statistics for Infrastructure Investment and Municipal Reform

	cities	years	observations
Number in sample:	144	23	3312

Number of cities in sample that:	CIVSER	MANAG	COMMISS
ever adopted	76	20	60
adopted during sample period	40	19	59*
dropped during sample period	1	3	14

*58 when the year 1904 is dropped from the sample

Variable	Mean	Std. Dev.
RSW	0.176	0.099
RS	0.138	0.083

Table 2b: Summary Statistics for Employment and Value-added Growth

	cities	5-year periods	observations
Number in sample:	141	5	705

Variable	Mean	Std. Dev.
EMPGR	0.0201	0.0546
VALGR	0.0700	0.1520

Table 3: Cross-section Regressions for Employment Growth 1904-1929

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
INTERCEP	-0.0201	0.0054	0.0015	-0.00001	-0.0131	0.0092	0.0009	-0.0023	0.0233
	(0.0046)	(0.0116)	(0.0121)	(0.0130)	(0.0045)	(0.0124)	(0.0129)	(0.0139)	(0.0145)
RSW	0.2185	0.2123	0.2141	0.2097					
	(0.0248)	(0.0245)	(0.0309)	(0.0314)					
RS					0.2278	0.2183	0.2175	0.2131	
					(0.0303)	(0.0304)	(0.0384)	(0.0387)	
LEMP99		-0.0027	-0.0023	-0.0024		-0.0024	-0.0015	-0.0014	-0.0016
		(0.0011)	(0.0012)	(0.0014)		(0.0012)	(0.0013)	(0.0014)	(0.0016)
MIDWEST			-0.0020	-0.0016			-0.001.5	-0.0015	0.0115
			(0.0036)	(0.0038)			(0.0039)	(0.0042)	(0.0038)
SOUTH			0.0025	0.0035			0.0046	0.0053	0.0097
			(0.0038)	(0.0042)			(0.0039)	(0.0043)	(0.0047)
WEST			0.0040	0.0037			0.0102	0.0097	0.0222
			(0.0058)	(0.0059)			(0.0058)	(0.0059)	(0.0060)
CIVSER				0.0023				0.0022	0.0024
				(0.0038)				(0.0039)	(0.0043)
MANAG				0.0105				0.0156	0.0185
				(0.0117)				(0.0122)	(0.0134)
COMMISS				0.0030				0.0043	0.0066
				(0.0046)				(0.0047)	(0.0052)
R-square	0.3593	0.3848	0.3943	0.4007	0.2889	0.3075	0.3366	0.3485	0.1984
Root MSE	0.0152	0.0149	0.0150	0.0151	0.0160	0.0159	0.0157	0.0157	0.0174

Dependent variable mean (std. dev.): 0.019 (0.019). Number of observations = 141. Variable definitions given in Table 1. Standard errors in parentheses.

Table 4: Cross-section Regressions for Value-added Growth 1904-1929

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
INTERCEP	0.0213	0.0572	0.051[illegible]	0.0438	0.0299	0.0607	0.0475	0.0371	0.0760
	(0.0062)	(0.0251)	(0.0262)	(0.0283)	(0.0060)	(0.0264)	(0.0279)	(0.0299)	(0.0316)
RSW	0.2389	0.2352	0.2759	0.2652					
	(0.0334)	(0.0334)	(0.042[illegible])	(0.0423)					
RS					0.2425	0.2365	0.2748	0.2664	
					(0.0405)	(0.0408)	(0.0525)	(0.0522)	
LVAL99		-0.0022	-0.002[illegible]	-0.0018		-0.0019	-0.0013	-0.0009	-0.0015
		(0.0015)	(0.0016)	(0.0018)		(0.0016)	(0.0017)	(0.0018)	(0.0020)
MIDWEST			-0.0072	-0.0070			-0.0[illegible]65	-0.0069	0.0095
			(0.00487)	(0.0052)			(0.0[illegible]53)	(0.0057)	(0.0051)
SOUTH			0.0005	0.0019			0.0[illegible]32	0.0043	0.0097
			(0.0052)	(0.0057)			(0.0[illegible]54)	(0.0059)	(0.0063)
WEST			-0.0086	-0.0090			-0.0[illegible]08	-0.0019	0.0137
			(0.0077)	(0.0077)			(0.0[illegible]78)	(0.0077)	(0.0078)
CIVSER				0.0036				0.0037	0.0043
				(0.0051)				(0.0053)	(0.0057)
MANAG				0.0267				0.0333	0.0365
				(0.0159)				(0.0164)	(0.0179)
COMMISS				0.0072				0.0087	0.0113
				(0.0061)				(0.0063)	(0.0069)
R-squared	0.2686	0.2800	0.2967	0.3177	0.2047	0.2129	0.2292	0.2605	0.1143
Root MSE	0.0205	0.0204	0.0204	0.0204	0.0214	0.0214	0.0214	0.0212	0.0231

Dependent variable mean (std. dev.): 0.064 (0.024). Number of observations = 141. Variable definitions given in Table 1.
Standard errors in parentheses.

Table 5: Panel Regressions for Investment Shares of Municipal Expenditure

Dep.Var.	RSW	RS	WATER	NONINFRA	RSW	RS	RSW	RS
CIVSER	0.0079	0.0147	-0.0068	0.0068	0.0075	0.0120	0.0076	0.0121
	(0.0069)	(0.0057)	(0.0044)	(0.0049)	(0.0065)	(0.0053)	(0.0065)	(0.0053)
MANAG	-0.0060	-0.0043	-0.0017	-0.0131	0.0001	0.0017	-0.0015	-0.0012
	(0.0099)	(0.0083)	(0.0063)	(0.0071)	(0.0091)	(0.0075)	(0.0097)	(0.0080)
COMMISS	-0.0177	-0.0161	-0.0016	0.0046	-0.0125	-0.0120	-0.0162	-0.0143
	(0.0059)	(0.0049)	(0.0038)	(0.0042)	(0.0055)	(0.0045)	(0.0062)	(0.0051)
RSW_{-1}					0.4265		0.4255	
					(0.0163)		(0.0163)	
RS_{-1}						0.4528		0.4517
						(0.0160)		(0.0160)
MANDROP							0.0187	0.0260
							(0.0248)	(0.0203)
COMDROP							0.0154	0.0092
							(0.0118)	(0.0096)
R-square	0.3375	0.3495	0.1919	0.2351	0.4610	0.4877	0.4614	0.4881
Root MSE	0.0827	0.0690	0.0523	0.0586	0.0744	0.0609	0.0744	0.0609
# Obs	3312	3312	3312	3312	3168	3168	3168	3168

City and time dummies included. Variable definitions given in Table 1. Standard errors in parentheses.

Table 6: Panel Regressions for Employment and Value-Added Growth

Dep. Var.	EMPGR	EMPGR	EMPGR	EMPGR	VALGR	VALGR	VALGR	VALGR
RSW	0.2998	0.3124			0.2712	0.2361		
	(0.0680)	(0.1226)			(0.2076)	(0.3154)		
RS			0.2616	0.2625			0.2175	0.1611
			(0.0718)	(0.1565)			(0.2254)	(0.3523)
LEMPINIT	-0.0025	-0.0034	-0.0022	-0.0043				
	(0.0019)	(0.0032)	(0.0019)	(0.0035)				
LVALINIT					0.0049	0.0102	0.0052	0.0104
					(0.0062)	(0.0094)	(0.0061)	(0.0095)
Includes City Random Effects	No	Yes	No	Yes	No	Yes	No	Yes

Time dummies included. Number of observations = 564. Variable definitions given in Table 1. Standard errors in parentheses.